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DATE: November 18, 1986

TO: Tilak Verma

FROM: Paul Davis

SUBJECT: COMMENTS ON THE BWIP PRE-MEETING OF NOVEMBER 5, 1986.

This memo is in response to your request for my comments on the topics covered in the BWIP pre-meeting held on November 5, 1986 at the Willste building in Silver Spring, Md. These topics, all of which are related to the calculation of a ground-water travel time (GWTT) for the BWIP site, are:

1) the sufficiency of data required for the calculation

2) the validity of BWIP conceptual models

3) Clifton's (1986) GWTT calculation

4) NWC' GWTT calculation

As the calculations mentioned above are dependent on both the data and the conceptual models, it is difficult to discuss the four topics separately. However, the conceptual models used by both Clifton and NWC are very similar and therefore that issue will be discussed first.

VALIDITY OF BWIP CONCEPTUAL MODELS

The conceptual model used by both NWC and Clifton is comprised of a single basalt flow top thought to be located above the repository. Flow within this unit is assumed to be two-dimensional for the Clifton analysis while NWC considers the flow to be one-dimensional. Both analyses assume that the flow top behaves hydraulically as a porous media. While the NWC analysis assumes their one-dimensional system to be homogeneous (it is by definition isotropic), Clifton's analysis treats the flow top as a heterogeneous, isotropic unit.

Both models are very idealistic representations of the system and therefore not truly "valid" conceptual models. However, the more relevant question is perhaps, how do the differences between these models and "reality" affect the predicted travel times.

Probably the main concern with the conceptual models is the assumption of

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porous media flow conditions. A significant amount of evidence exists that indicates that fractures or thin zones of high permeability material may control the movement of water in the flow tops. This evidence includes physical descriptions of the drill core, geophysical logs, hydraulic responses (both single hole tests and responses to drilling), and even perhaps the only tracer test that was conducted at the BWIP site. With respect to the NWC and Clifton calculations, the main problem is that the hydraulic parameters used to define the sampled distributions may not be the parameters that control transport through the flow tops. For example, if large scale channeling within flow tops occurs, as indicated by the hydraulic responses to the drilling of DC-23, then only hydraulic conductivities obtained from within the channels should be used in the analysis. On the contrary, both Clifton and NWC use conductivities obtained from zones of both high and low conductivity and weight their analyses toward the zones of low conductivity.

A second concern arises from the assumed dimensionality of the problem. That is, neither analysis allows for vertical flow within the flow top or from the flow top through a flow interior and to another flow top. Neglecting vertical flow within a flow top may not be of serious concern if the hydraulic parameters have been obtained from tests of the correct interval. However neglecting flow between flow tops could lead to serious error. Both Clifton and NWC seem to believe that including vertical flow would only lead to more "conservative" results. However that is due to the unrealistic way they have treated the flow tops. Specifically, they have assumed that the range of a parameter for their idealistic flow top is represented by the range of that parameter taken over all flow tops. For example, the range and distribution of transmissivities has been obtained from data on all flow tops. Therefore, any variation between flow tops has been neglected. The concern over flow between flow tops would come (in a more realistic analysis) if water moved from a flow top that had a lower transmissivity, through a flow interior, and to a flow top that had a higher transmissivity. Unfortunately, how conservative or non-conservative this type of analysis would be can not be determined until a better understanding of flow through the interiors is obtained.

CALCULATIONS OF GROUND-WATER TRAVEL TIME

I provided Neil Coleman with a detailed review of the Clifton (1986) work that I wrote on June 15, 1986. Therefore, this memo does not contain such a review. Instead, it contains general comments about Clifton's and NWC's general approach to the calculation of a ground-water travel time. Prior to discussing these calculations I would like to say a word about the appropriateness of including a calculation of GWTT in a review of a paper on GWTT calculations, as was done in the case of NWC's review of Clifton's paper. Although because of our technical backgrounds we find it almost impossible to resist such an effort (NOTE: I provided NRC with such a calculation of GWTT in my review of the draft BWIP EA), I believe it is inappropriate. This is because I perceive the role of the reviewer to be one of commenting on the original work not one of redoing the work. For example, if I were to review an article on a particular aquifer test my role would be to point out problems or limitations of the author's interpretation of the data. My role would not be to re-analyze the test and tell the author what I thought the correct transmissivity should be. If I felt the author's transmissivity value was wrong, then I could publish my own interpretation of the data (which, of coarse, the previous author could then review). In the case of GWTT calculations for BWIP, the NRC has a need for independent evaluations but the place for those is not in a the review of DOE work. As independent evaluations, the NRC would have the added benefit of being able to elicit comments from the entire technical community on NRC's approach to the GWTT calculations.

BWIP GWTT Calculations

In my opinion, the NWC and the Clifton calculations different mainly in their approach to the GWTT issue. That is, I would regard the NWC analysis as some kind of bounding calculation while I believe Clifton's would be regarded as a calculation which is somewhat more representative of real site conditions. Let me say, however, that I don't know if NWC intended to produce a bounding case analysis but due to the one-dimensional nature of their problem, their approach is much more of a bounding case than that of Clifton.

Initially, I would like to discuss the bounding type of calculation. This discussion has two parts: 1) can NWC's analysis be considered a bounding calculation; and 2) how I perceive the use of a bounding calculation in licensing activities. First of all, I don't believe it is really a bounding calculation because of the assumptions about the types of probability density functions used to describe the transmissivity and hydraulic gradient. While the DOE has done some statistical tests to justify the distribution used for the transmissivity, they included transmissivities for all flow tops and therefore did not establish the justification of the assumed distribution for the flow top above the repository. In addition, the assumption of a log-normal distribution for hydraulic gradients is unsupported by site data or by any other investigations on hydraulic gradients. Nevertheless, the NWC results indicate the NRC requirement on GWTT is violated therefore there is really no point in further discussion of how bounding an analysis NWC has performed. This brings me to the second part of the discussion. That is, given that a bounding type of analysis has is performed, what is its relevance to licensing activities. From my point of view the usefulness of the analysis depends on the results. For example, if an extremely conservative bounding case analysis indicated that the travel time was well in excess of 1,000 years, then further work on calculating GWTT's is unwarranted and with respect to this criterion the site could be licensed. On the other hand, if, as in the case of the NWC analysis, the bounding case indicates that the site violates the GWTT criterion then more realistic analysis must be performed. I believe this approach is necessary to avoid disqualifying a site for which the "true" GWTT is greater than 1,000 years and where the bounding calculations by there very nature do not represent realistic conditions.

Clifton's analysis is based on a more realistic model which allows for a heterogeneous flow top. However, as I pointed out previously, the assumptions on probability distributions and spatial correlations are not supported by the existing data and the manner in which the analysis has been done yields results that are inconsistent with the observed behavior of the system (note: this is even more true for the NWC analysis). These assumptions and the approach to the analysis may lead to travel times which are longer than the "true" travel time.

The remaining question is then; given that bounding calculations yield travel times that violate the NRC criterion, what is the most realistic analysis of GWTT that can be supported with the existing data? To answer that question I have made the following list of conditions that I believe should be required of any model used to calculate GWTT:

1) Any model used to predict ground-water travel times should be consistent with all available site data. In particular, this means that the model should be forced to apply the measured transmissivities and hydraulic heads at their measurement locations. Not only is it misleading to ignore the knowledge about the distribution of parameters, in the analysis performed so far, it also adds uncertainty to the results. A perhaps more stringent requirement (that I also believe would lead to reduced uncertainty) would be to force the model to be able to recreate the hydraulic responses that have been observed at the site. One final point, the model should also be consistent with the observed geochemical evolution of the ground water and ages of ground-water interpreted from isotopic composition.

2) In regions that lack data, minimal assumptions about nature of the parameters in those areas should be made. For example, all of the spatial correlation structures and lengths (with the exception of the zero correlation length) assumed by Clifton are unsupportable with the existing data. Additionally, the log-normal distribution for transmissivities assumed by both Clifton and NWC is not supported by the data. Therefore, with respect to transmissivities, one is forced to assume that regions without data could have any value of transmissivity that has been measured for that flow top. This also implies that samples of transmissivity values for these regions would be derived from the measurements themselves, not from an assumed distribution (i.e. log normal). The approach may differ for hydraulic heads, as some spatial correlation structure and corresponding correlation length can be defined for many parts of the flow system. The definition of this structure would help reduce uncertainty in the estimated GWTT. Finally, with respect to the effective porosity (or effective thickness, depending on the analysis) were only one data point exits, I feel the only supportable approach is to assume that all flow tops at all locations have effective porosities equal to that value. Thus, no assumed distributions should be used, nor should any correlations (either spatial or between variable, say porosity and hydraulic conductivity) be assumed. This is because not only have no such distributions or correlations been defined for the BWIP site, no such distributions or correlations have been defined for basalt flow tops anywhere.

In my opinion, adhering to the above conditions would yield GWTT estimates that are supported by the existing data. However, this does not guarantee that the "true" travel time will be predicted because of the lack of data (especially with regard to the effective porosity) and because of the large uncertainty in conceptual models.